## *Addition to the Paper on the Number of distinct Terms in a Symmetrical, or partially Symmetrical Determinant.*

By Professor Cayley.

The generating function

$$u_x = 1 + u_1 x \ldots + u_n \frac{x^n}{1.2 \ldots n} + \ldots, = \frac{e^{\frac{1}{2}x + \frac{1}{4}x^2}}{\sqrt{1-x}}$$

was obtained as the solution of the differential equation

$$2\frac{du}{dx} = u\left(1 + x + \frac{1}{1-x}\right).$$

Writing this in the form

$$2(1-x)\frac{du}{dx} = n(2-x^2),$$

we at once obtain from $u_n$ the equation of differences,

$$u_n = n u_{n-1} - \tfrac{1}{2}(n-1)(n-2) u_{n-3};$$

and it thus appears that the values of $u_n$ (number of distinct terms in a symmetrical determination of the order $n$) can be calculated the one from the other by the process

| | |
|---|---|
| $n = 1,$ | $1 = 1.1$ |
| $= 2,$ | $2 = 2.1$ |
| $= 3,$ | $5 = 3.2 - 1.1$ |
| $= 4,$ | $17 = 4.5 - 3.1$ |
| $= 5,$ | $73 = 5.17 - 6.2$ |
| $= 6,$ | $388 = 6.73 - 10.5$ |
| | &c. |

which is one of extreme facility.

---

## *Continued Observations of the Companion of Procyon.**

By Herr Struve.

(*Translation.*)

The months of February and March were this year singularly unfavourable for Astronomical observations. In the few hours which were free from clouds, the images were for the most part so tremulous, that micrometrical measurements even of objects of comparatively little difficulty could scarcely be undertaken. Not until the 21st of March did I succeed in obtaining

* The substance of this paper was delivered orally by M. Struve, at the evening meeting, on May 8. The paper, which is written in German, has been kndly translated by W. T. Lynn, Esq., B.A.—[Ed.]

a tolerably satisfactory measure of the companion of *Sirius*. Although on that evening the images were still not of the best, yet I could not neglect the opportunity of at once examining *Procyon* in reference to the small point of light which I had remarked in its neighbourhood last year. On looking into the telescope accordingly on that night, I thought that I recognised by glimpses such an object with some certainty, of which I estimated the position-angle at about 95°; and my assistant, Herr Lindemann, succeeded in making a determination agreeing within 0°·1 with my approximate one, unknown to him when making his. Immediately after this the images became so tremulous again, that the point of light could no longer be perceived. On the 1st of April the images were better, but streaks of nebulous cloud made continuous observation impossible. During some short intervals free from cloud, we succeeded in making only three separate hasty measures of the position-angle, the mean of which gives 93°·3: a determination to which, on account of the haste with which it was necessarily made, I should only attribute a small degree of accuracy.

The following week the sky was again constantly clouded. Not till the 9th of April could I obtain the first complete measurement; after which three others, on April 13 to 15, were made, under very good atmospherical circumstances. The observations were all made with the Power III. of 309 times. Each position-angle below depends upon three determinations for the distances; the number of measures is affixed to each in parentheses.

| Date. | Time of Obs. | Distance and | No. of Measures. | Position of Instrument. | Time of Obs. | Position-Angle. | Position of Instrument. |
|---|---|---|---|---|---|---|---|
| | h m | ″ | | | h m | ° | |
| 1874 April 9 | 9 40 | 10·47 | (2) | I. | 9 5 | 97·0 | I. |
| | | | | | 9 30 | 94·8 | I. |
| April 13 | 9 35 | 12·17 | (3) | I. | 9 28 | 96·6 | I. |
| | | | | | 9 40 | 94·5 | I. |
| | | | | | 9 54 | 98·0 | II. |
| April 14 | 9 50 | 12·04 | (2) | II. | 9 38 | 94·9 | II. |
| | | | | | 10 1 | 99·4 | II. |
| April 15 | 9 40 | 12·00 | (3) | I. | 9 33 | 92·8 | I. |
| | | | | | 9 46 | 97·9 | I. |

Besides these, Herr Lindemann made the following measures of the angle of position:—

| | ° |
|---|---|
| April 9 | 99·3 |
| „ 13 | 94·8 |
| „ 15 | 96·6 |
| Mean | 96·9 |

In regard to the measures of distance, the agreement on the three last evenings has been remarkably favoured by fortuitous circumstances, for the separate determination on those evenings differ from each other by a whole second and more, as might indeed be expected from the difficulty of the object. It is not, therefore, surprising that the distance on the 9th of April should be smaller by 1″·70. Last year the differences on separate evenings were considerably greater.

To my observations the systematic corrections derived from measures with artificial double stars must be applied. These are for the distances quite insensible, since they amount in the case before us to only 0″·01 at the maximum. For the position-angles, on the other hand, they are considerable. Subject to small variations for the different hour-angles, they amount at the mean to 2°·95.

I would here remark, that in my paper of last year on the amount of the systematic correction, an error has been overlooked. It amounts for last year in the mean for Power III. to + 2°·66 and for Power IV. to 1°·61.

Now, if we take the mean values from my observations for the two years, we have

| | | | |
|---|---|---|---|
| 1873 | March 28 | $d = 12''.49$ | $P = 87°.65$ |
| 1874 | April 10 | 11·67 | 96·65 |

or, after applying the systematic corrections:

| | | | |
|---|---|---|---|
| 1873 | March 28 | $d = 12''.49$ | $P = 90°.24$ |
| 1874 | April 10 | 11·67 | 99·60 |

The distance in the interval would, therefore, seem to have diminished by about 0″·8. But owing to the difficulty of the measures, the mean values themselves must be subject to such uncertainties that the reality of the apparent diminution must be considered doubtful. But in regard to the increase of the position-angle, there can scarcely be any doubt. Not only does the observed increase of 9°·5 correspond to a considerable linear change of place, amounting to 2″·0, but the measures of direction are in themselves much easier, and more certain than those of distance.

It is well known that Professor Auwers, as soon as he had received my observations of last year, repeated his investigations into the variable proper motion of *Procyon*, availing himself also of the observations of this star which has been made since 1862. From this he concluded that it was doubtful whether the object observed by me was really the sole body disturbing the proper motion of *Procyon*, but that the doubt would be removed if it appeared this spring that the position-angle had undergone an increase of from 9° to 10°. And this increase has really shown itself above in the most remarkable manner. I consider it,

therefore, to be decisively established that the object I have observed is actually the companion whose existence has been theoretically proved by the calculations of Auwers; and hope that the astronomical world will rejoice with me in the triumph thus obtained for the labours of my honoured friend, and through them for our common science. In order to remove any exception that might be taken that the wished-for result had in any degree been itself the cause of the recognition, and affected the measurement of the place of so difficult an object, I will just remark, that I had not looked again at the paper of Auwers in question since its first receipt last summer, and had totally forgotten the data of its criterion, and the mutual relation of the two stars. I did not again take it up until after I had succeeded in making the first observation, and the results of that paper were even less present to the mind of my assistant, Herr Lindemann, whose younger eye appears generally to have seen the companion even better than mine.

On the other hand, we cannot lose sight of the important fact that various other astronomers, using very powerful instruments last winter and spring, have, so far as has hitherto been known to me, sought in vain for the faint companion. In particular, this has been the case with the new refractor of the Washington Observatory of 26 inches aperture, with the performance of which Professor Newcomb expresses himself as well satisfied. Of the cause of the failure I can, not being in possession of detailed information concerning the circumstances of the observations, merely form conjectures. Perhaps the atmospheric conditions during the Washington observations were less favourable than those here; perhaps also the surpassing brilliancy of the principal star may have hindered the recognition of the small companion in its neighbourhood, if the observer made use of the whole aperture of 26 inches. How great is the effect of the brilliancy of the principal star has shown itself very clearly in our observations. Although the near companion is doubtless considerably brighter than the known little star, which is near *Procyon* without belonging to its system, and precedes it about 42″ to the north, the latter could always be seen in a dark night with ease by all observers who could not see the former at all, which was best seen in feeble twilight, whilst the more distant little star was not visible at all. But perhaps, also, practice in the recognition of such objects, and the greater or less sensibility of the retina, may have had their influence. In this respect also our observations have furnished some remarkable facts. In the last favourable observing evenings I requested six other astronomers to look successively at the interesting object. But whilst Herr Lindemann and myself saw and measured it with comparative ease; of the six in question only one—Herr Ceraski, second Assistant at the Moscow Observatory—succeeded in seeing it with certainty without being warned of the place where it was to be looked for. Of the others some thought they perceived it after

its place had been accurately pointed out to them, but not with satisfactory certainty. I myself always made the observations best if I came to the telescope with fresh eye. After some minutes the eye became usually so exhausted that I was obliged to give it some rest to enable me to see the object. Some of the later observations on separate evenings are therefore of inferior accuracy.

The difficulty of the observation of the companion of *Procyon* will of course be unequally greater than that of the companion of *Sirius*. In our latitudes this difficulty is somewhat equalised by the low position of *Sirius*: for the companion of which I have looked in vain on many evenings in the years immediately after its discovery by Alvan Clark, until at last favourable atmospheric conditions enabled me to see it distinctly. But after I had once recognised it, and thereby followed the modifications of its appearance with tremulous images, it is now only when there is a high degree of tremor that I cannot see it, at least by glimpses, with certainty. These experiences have doubtless been of use to me in the recognition of the companion of *Procyon*, and give me also full confidence in the accuracy of my perceptions, independently of the corroborating observations of Lindemann and Ceraski. Notwithstanding this, however, I willingly acknowledge that a confirmation of them by other observers at other places with other instruments, would be very welcome to me.

---

## *On a Method of finding the Parallax of Double Stars, and on the Displacement of the Lines in the Spectrum of a Planet.*

By Professor C. Niven.

(*Communicated by W. H. M. Christie, Esq.*)

§ 1. The idea of Doppler that the motion of the fixed stars towards or from the Earth must exercise an influence on the nature of the light which we receive from them, though it has not served his purpose of explaining their colours, has yet not been without fruit. For although, in general, no great change of colour can be looked for in this direction, we may nevertheless expect to find traces of the effect in a displacement of the fixed lines of the spectrum. The displacement of the line F of hydrogen for a velocity of 20 miles per second amounts to nearly $\frac{1}{12}$th of the distance between the constituents of the double line of sodium, and is towards the violet or red end, according as the star is approaching or receding from us. Now displacements of this magnitude are fully within the reach of modern instruments, and accordingly Mr. Huggins has succeeded in estimating the velocities of separation or approach of a number of the brighter stars.